

Review of the bachelor's thesis entitled An Investigation of Effort-Reward Imbalance and Cognitive Inhibition in the Context of Higher Education by Jackson David Ellison

This thesis is an empirical psychological study which tests the relation of performance in the Stroop test with a number of variables related to health and stress on a sample of 39 students of the Faculty of Humanities, Charles University.

The introductory parts of the thesis deal with stress and other related concepts as uncontrollability and effort-reward imbalance. Furthermore, the author describes the concept of cognitive inhibition as a hypothetical key factor among characteristics belonging to executive functions, with respect to an association with psychological well-being and health. He shows a fair knowledge of literature in the field, and presents the relevant concepts clearly. Because a bachelor's thesis offers enough space for a thorough description of basal concepts, I would personally appreciate an even wider discussion of the cognitive concepts in question. For example, how is cognitive inhibition (or inhibition) related to attention? What is the theoretical relationship between effort and attention? Could higher effort (with constant reward) be responsible for the positive relationship between effort-reward imbalance and cognitive inhibition?

Aims of the study are slightly less clear. The author speaks about mediation of the relationship between uncontrollability and depression by cognitive inhibition, but a mediation model is not performed or at least proposed. It turns out, that only the relationship between effort-reward imbalance and cognitive inhibition is tested in the first step. In the second step, correlates of depression are sought among several stress factors. I appreciate that the author carefully explains the choice of measures for respective concepts. He brings many arguments, mainly of methodological kind, for such choices. However, my general impression from this study is that a lot of hypothetical propositions had to be made on the way from what is known from literature (such as that work stress is associated with higher incidence of depression), the basic theoretical argument of the study (which I would say, is that reward might cause depression via impaired cognitive inhibition), and finally to the actual measures performed (a modified effort-reward questionnaire for Chinese students, Stroop test and self-reported health and control). I think that the concepts and measures should be carefully distinguished and great attention paid to every switch from one to another. For example, bringing evidence that stress relates to executive functioning does not necessarily mean that controllability will be also associated with executive functioning because there are other aspects to stress but controllability.

Some specific notes to methodology: It is praiseworthy that the author explains the omission of the overcommitment component of the Effort-Reward Imbalance Questionnaire. The paragraphs dedicated to this topic, however, could be more clear and brief. Authors and citations of Czech translations of questionnaires should be given. In addition, for a better readers' idea about the operationalized variables, the questionnaire that were used should be attached, or at least example items given. Linear regression model should not include variables which are strongly intercorrelated. This is the case of Patient Health Questionnaire (PHQ-9), the General Health Questionnaire (GHQ-12), as well as the GHQ-12 and the measure of anxiety, and perhaps other variable pairs.

In general, the author has done a good job performing and presenting the analyses. He handled several statistical methods, which he clearly described. Results are presented in a suitable concise way. However, given the small sample size and a large number of variables involved, the overall model gets somewhat unreliable. Moreover, the problem of a somewhat weakly argued for main hypothesis leads to a difficulty to decide to which level (if theoretical derivation of the variables, choice of measures, scoring, small sample size etc.) a failure to confirm one of the hypotheses should

be ascribed. This might be a reason why in discussion the author brings arguments for such a failure which do not seem to hit the core of the problem. The first argument is about problematic scoring of Stroop test, which the author refuses at the end, and the second speaks about a lack of participants with depressive symptoms in the sample. While sample of students obviously differs from clinical samples, I am not sure if this can be used as argument for a failure to confirm a proposed negative relationship between a stress variable (effort-reward imbalance), and cognitive variable (cognitive inhibition). Should not this hypothesized relation, stated as it is, be valid in any sample? How can be previous studies on the association between Stroop test results and depression relevant when explaining the surprising zero finding? To name some of the positive aspects of the discussion section, it shows the author's awareness of the methodological limits of his study, and his ability to compare own results with literature sources.

In summary, the proposed thesis is a nice work which approves the ability of the author to perform all parts of an academic study, in particular reviewing literature, designing a study, collecting data, performing analyses and discussing their findings. I see the reading and writing parts as strong, and the experimental part as clearly sufficient for obtaining a bachelor degree. I think this thesis should be marked as excellent.

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